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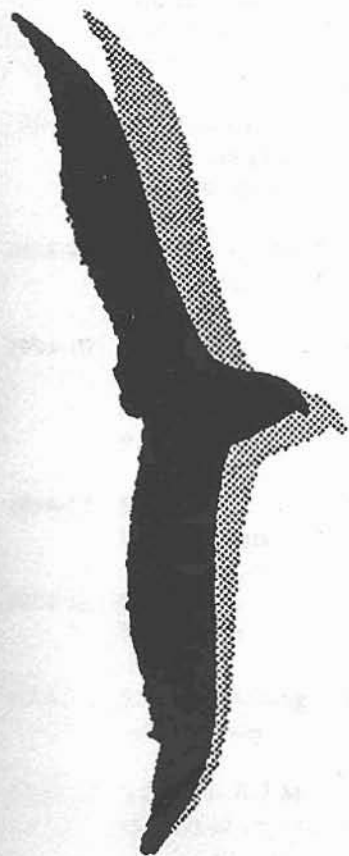
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Jaap H. Abbring
Gerard J. van den Berg
Jan C. van Ours

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Jaap H. Abbring *
Gerard J. van den Berg †
Jan C. van Ours ‡

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Abstract

This paper analyzes the effect of unemployment insurance sanctions on the transition rate from unemployment to employment. Sanctions are punitive benefits reductions that are supposed to make recipients comply with certain minimum requirements concerning search behavior. We use a unique set of administrative micro data covering the whole population of individuals who started collecting unemployment insurance in the Netherlands in 1992. To deal with the selectivity of the occurrence of a sanction we simultaneously model the process by which unemployed get a sanction and the process by which they find jobs. We exploit the fact that some respondents experience multiple spells.

*Department of Economics, University of Amsterdam, Roetersstraat 11, NL-1018 WB Amsterdam, The Netherlands, and Tinbergen Institute.

†Department of Economics, Free University Amsterdam, De Boelelaan 1105, NL-1081 HV Amsterdam, The Netherlands, CEPR and Tinbergen Institute.

‡Tinbergen Institute, Department of Economics, Erasmus University, P.O. Box 1738, NL-3000 DR Rotterdam, The Netherlands.

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1 Introduction

In the Netherlands, if a worker loses his job, he is usually entitled to unemployment insurance benefits. However, in order to receive these benefits, the unemployed worker has to oblige certain rules concerning search behavior and registration. If a benefit recipient does not act according to these rules then a sanction may be applied, i.e. his benefits may be reduced for some period of time. Frequently used reasons for giving a sanction include: lack of action to prevent job loss, an insufficient number of applications in a given time period, rejecting suitable job offers, not providing enough information on search activities to the administrative agency responsible for the benefits payment, and simple lack of cooperation with this same institution. A sanction is given with the intention to change the behavior of the benefit recipient so that in the future he will act according to the rules. Since these rules are supposed to stimulate unemployed individuals to search actively for jobs, and since a benefit reduction by itself probably makes unemployed individuals more prone to accept jobs and to search more intensively, sanctions are expected to increase the exit rate out of unemployment into employment.

In the past decade there has been a change in the public debate with respect to the aims of social security. Whereas until the beginning of the 1980s the primary aim of social security was considered to be income support, in the late 1980s there was a shift towards stimulating benefit recipients to find jobs. It was commonly felt that the Dutch social security framework imposed a heavy burden on the economy, and that a strong policy towards abuse was necessary to maintain support for at least the essential features of this framework. This change in focus led to a policy change with respect to the application of unemployment insurance sanctions. Because of this, the number of sanctions increased substantially, both in absolute terms and as a percentage of the number of individuals flowing into unemployment insurance. Whereas, over the period 1987–1994, the annual inflow into the unemployment insurance benefit system doubled from about 300,000 to about 600,000, the number of unemployment insurance benefit sanctions increased from 27,000 to 104,000.

This paper investigates whether sanctions have an effect on the transition rate from unemployment to employment. We analyze micro unemployment duration data covering the whole population of individuals who started collecting unemployment insurance benefits in the Netherlands in 1992. The data are collected from administrative records. The advantage of this is that there is no selective nonresponse or attrition of individuals from the data set, and that there are no

initial conditions problems. The disadvantage is that the number of explanatory variables is not as large as in typical unemployment duration studies. However, since the data cover the whole population, we can estimate models for different subpopulations, thus allowing for maximum interaction between explanatory variables. Because the policy towards non-compliance of the rules is likely to differ between the institutions dealing with different segments of the labor market, it is particularly important to estimate models separately for different segments.

It is clear that the reduction in benefits by itself is a potentially important determinant of the exit rate out of unemployment into employment. The relation between unemployment benefits and the duration of unemployment has been one of the most frequently addressed issues in the literature on unemployment. There is an abundant empirical literature in which the effect of the benefits level on the average unemployment duration is identified by comparing the unemployment durations of individuals with different benefits levels (see e.g. the surveys in Layard, Nickell and Jackman (1991) and Devine and Kiefer (1991)). Much of this empirical literature assumes the benefits level and its effect to be constant in a given spell of unemployment. Also, the benefits variable is taken to be truly exogenous and uncorrelated with unobserved explanatory variables. In other words, when the benefits level of a given individual differs from the level of an individual with identical observed characteristics, then this is assumed to be unrelated to the unobserved differences between these individuals or their environment that influence the exit rate out of unemployment.

In addition to the approach described in the previous paragraph, two alternative approaches have been used to identify benefits effects. Van den Berg (1990a) structurally estimates a nonstationary job search model that takes account of the facts that the unemployment insurance (UI) entitlement period is finite and that the level of social assistance benefits received after this period is generally lower than the UI level. The latter are institutional features of the Dutch benefits system. In this model framework, changes in the behavior of a given individual over his spell of unemployment aid in identifying the effect of the benefits level on the exit rate out of unemployment. Van den Berg (1990a) assumes that individuals anticipate future changes in the benefits level. The model does not allow for unobserved heterogeneity. Meyer (1990) estimates a reduced form model of unemployment duration, taking account of limited benefits entitlement. The benefits effect is mainly identified by comparing durations of individuals with different benefits levels. However, because in his sample the main variation in benefits originates from the fact that the unemployment benefits system differs across different US states, his results are less vulnerable to the possible endogeneity of

the benefits level than results from other studies.

It is clear that a sanction is not merely a reduction in benefits. The sanction is induced by a failure to oblige certain rules concerning search behavior, and the administrative agency responsible for benefits payment will motivate its decision to the individual. Furthermore, as will be shown below, the individual has an incentive to comply with these rules for the remainder of the UI entitlement period. All this is likely to increase the search intensity of the individual from the moment at which the sanction is imposed onwards. There is evidence that an increase in search intensity increases the transition rate from unemployment to employment (see Devine and Kiefer (1991) for a survey). Recently, two studies have appeared on the effect of having UI recipients be interviewed by officials of the UI agency or the employment office (Gorter and Kalb (1996) and Dolton and O'Neill (1996)). These studies are unique in that they use data from controlled social experiments, from the Netherlands and the U.K., respectively. In both cases, the interviews are supposed to provide advice and counseling to the unemployed individual. Both find a significant and lasting effect on the transition rate from unemployment to employment.

In the context of the present paper, the endogenous selection involved in the imposition of sanctions is the main issue to be addressed in an empirical analysis. Sanctions are imposed in response to the behavior of the unemployed individual. To be precise, employees of social security councils decide to impose or not to impose a sanction to a given individual on the basis of objective information and subjective evaluation of the individual's behavior. Individuals on which sanctions are imposed are most likely different (in ways that are unobserved to the researcher) from other individuals. If the endogenous (selective) imposition of sanctions is not taken into account then the estimate of the sanction effect will be biased. For example, if individuals who often get punished by a sanction on average have unobserved characteristics that enable them to find a job relatively fast, and if this is not taken into account, then the effect of sanctions will be over-estimated. We should note that, unfortunately, the data do not define a natural experiment. We will return to this issue in Section 3.

Recently, numerous empirical studies have been published focusing on the impact of government interventions on individual labor market spells, notably on the impact of training programs on the duration of unemployment. In this literature, the main empirical problem usually concerns the possible endogeneity of training, or, in other words, the possibility that enrolment into the program is selective. In the case of non-experimental data, a popular approach to this problem is to model both the process by which unemployed enter a training

program and the process by which they leave unemployment, and to allow these processes to be dependent by way of their unobserved determinants, in addition to a direct effect of training on exit out of unemployment (see e.g. Heckman (1993) for a survey, and Gritz (1993) and Bonnal, Fougère and Sérandon (1994) for analyses using duration models).

In the present paper we use a similar approach. We specify the joint distribution of unemployment durations and the durations until the imposition of a sanction, and we model these durations to be dependent by way of (i) related unobserved determinants, and (ii) a direct effect of a realized sanction on the exit rate out of unemployment into employment. The second effect is the “parameter of interest”. It is present even if there are no correlated unobserved determinants.

We allow the rate at which a sanction is applied (i.e. the hazard rate of the distribution of the duration until a sanction) to depend on (i) observed explanatory variables, (ii) the elapsed unemployment duration and (iii) unobserved determinants (we use a Mixed Proportional Hazard (MPH) specification). For the duration dependence we take a flexible piecewise constant specification. We expect the rate at which a sanction is applied to increase shortly after the inflow into unemployment, since a certain amount of information has to be collected by the institution responsible for UI payment before a sanction can be justified. The exit rate out of unemployment into employment is modeled in a similar way, with the qualification that one of its explanatory variables depends on the actual state of the sanction process. The latter is a time-varying explanatory variable that is not independent of the unobserved determinants.

In addition to this, the present paper contains some theoretical and methodological innovations. As such, the paper provides a general framework for the analysis of punitive actions in social security systems. In Subsection 2.1 we provide a job search model in which a UI system with sanctions is integrated. This model is used to generate a number of predictions on the effect of a sanction on the transition rate from unemployment to employment, as well as on the differences between a world with a UI system that allows for sanctions and a world with a UI system that does not allow for sanctions. The job search model also clarifies the endogenous selection issues in non-experimental data.

In Subsection 2.3 we design a relatively simple *graphical* method to obtain evidence on the presence of a sanction effect. Basically, the consecutive timing of sanctions and exits out of unemployment is very informative on the magnitude of the sanction effect. It should be stressed that this graphical method is fairly general and can be applied to other analyses of the effect of one random event on the other, in the presence of selectivity.

The data we use contain respondents with multiple unemployment durations. As may be clear intuitively, such data greatly facilitate the identification and estimation of duration models (see Honoré (1993) and Heckman and Taber (1994) for a number of results on the identification of duration models using multiple spells, and Gritz (1993) and Bonnal, Fougère and Sérandon (1994) for empirical analyses of more or less similar multivariate duration models using multiple observed spells per duration). We show that in an ideal situation (no right-censoring), multiple spell data are very informative on the magnitude of the sanction effect.

The outline of the paper is as follows. In Section 2 we present the theoretical job search model, the multivariate duration model, the graphical checks, and we discuss some identification issues. Section 3 gives a detailed description of the Dutch unemployment insurance system and the data. Section 4 discusses estimation of the multivariate duration model. In Subsection 4.1 the likelihood of the model is derived. In Subsection 4.2 the estimation results are presented. In Subsection 4.3 we perform sensitivity analyses, we test the model specification, and we estimate extended model versions. In Section 5 we use the estimated model to compute summary measures of the unemployment duration distribution and the sanction process. Section 6 concludes. The Appendix provides more details on the data set.

2 The Model

2.1 A Job Search Model with Sanctions

In this subsection we present a job search model that incorporates punitive UI sanctions. This is a model of optimal behavior of unemployed individuals given the presence of a particular UI system in which sanctions can be imposed. The model helps to understand the effects of such a system on individual behavior. It also provides insights into the determinants of the rates at which jobs are found and sanctions are imposed and the relationships between these rates.

The point of departure is a basic job search model with endogenous search intensity, as presented by e.g. Mortensen (1986) and Albrecht, Holmlund and Lang (1991). Consider an unemployed individual who searches sequentially for a job. Given a particular search intensity s , job offers arrive according to the rate $\lambda(s)$, with $\lambda(s)$ increasing in s . The individual is able to choose s . Offers are random drawings from a wage offer distribution $F(w)$. Every time an offer arrives the decision has to be made whether to accept it or to reject it and search

further. Once a job is accepted it will be held forever at the same wage. During unemployment, a flow of benefits b is received and a flow of search costs $c(s)$ has to be paid, with $c(s)$ increasing in s , and with b possibly including a non-pecuniary utility of being unemployed. The individual aims at maximization of the expected present value of income over an infinite horizon.

It is well known that in this model the optimal strategy of unemployed individuals can be characterized by a reservation wage ϕ (giving the minimal acceptable wage offer) and an optimal search intensity s . Now let us introduce sanctions in this model framework.

It is useful to distinguish between sanctions as an institutional aspect of the environment of the individual, and the actual imposition of a sanction for an individual. Concerning the former, one may argue that the mere threat of a sanction should suffice to prevent it from ever being enforced. It is clear that the data contradict such a view. Alternatively, one may argue that the occurrence of a sanction is perfectly foreseen by the benefits recipient and is taken into account in determining his choices. The data and the results of this paper as well as institutional aspects of the UI system contradict this view as well. The moment at which a sanction is given may differ substantially across different unemployment spells of a given individual. Furthermore, there are occupational and regional (institutional) differences with respect to the rate at which sanctions are applied. (In Section 3 we discuss the institutional setting in detail.) There is variation across individuals and across unemployment spells in the strictness with which the rules are applied, and presumably there is a certain degree of randomness in this (this is confirmed in oral field research; see In 't Groen and Koehler (1993)). We conjecture that the individual does not exactly know the rules that he has to comply with, and that he does not exactly know what type of behavior will generate a sanction, or when it will be imposed. It is however plausible that the individual does know the relation between his behavior and the probability that a sanction will be imposed. Some individuals will be more willing to take the risk of being given a sanction than others (e.g. because they have a higher non-pecuniary utility of being unemployed).

In our stylized model, we therefore assume that, for an unemployed individual who has not yet been punished, there is a rate $p(s)$ at which a sanction is imposed, with $p(s)$ decreasing in s . For the moment we make the simplifying assumption that there is a threshold value s^* such that

$$p(s) = \begin{cases} p_0 > 0 & \text{for } s < s^* \\ 0 & \text{for } s \geq s^* \end{cases} \quad (1)$$

It may actually be realistic to have $p(s) = 0$ for all s exceeding a certain threshold

value. This is because any sanction policy needs to be backed up by explicit rules, and individuals can appeal against sanctions.

The individual does know p_0 and s^* but he does not know in advance when a sanction is imposed. So, having a high search intensity has the additional advantage that the probability of getting a sanction is low. In order to model the degree in which this is an advantage, we have to be specific about what happens after the imposition of a sanction. First of all, b is reduced substantially. Now if the individual persists in non-compliance after the imposition then in general additional benefits reductions are imposed. We assume that the punishment for such non-compliance is so severe that the individual will avoid this at all cost, so we take $s \geq s^*$ after imposition of a sanction. This implies that sanctions are imposed at most once in a given spell of unemployment. (A strategy in which individuals take a job upon imposition of a sanction, and quit immediately in order to make a "fresh start" in unemployment insurance, would not be optimal: UI would be reduced again immediately after quitting because of "lack of action to prevent job loss" (see Section 3).)

For the moment we make some additional simplifying assumptions. First, $\lambda(s) = \lambda_0 s$ and $c(s) = \frac{1}{2}c_0 s^2$. Secondly, the structural parameters b_1 (being the benefits level before a sanction is imposed), F , λ_0 , c_0 , p_0 , s^* and the discount rate ρ are constant as a function of unemployment duration. Upon imposition of a sanction, b is permanently reduced from b_1 to b_2 , with b_2 constant as a function of unemployment duration. The latter assumptions imply that both within the time interval before a sanction and within the time interval after a sanction, the optimal strategy is constant over time.

Let R_1 and R_2 denote the expected present value of income before and after imposition of a sanction, respectively, let ϕ_1 and ϕ_2 denote the corresponding reservation wages, and let s_1 and s_2 denote the corresponding search intensities. Using the familiar returns-to-assets representation of Bellman's equation (see e.g. Van den Berg (1990a)), we have

$$\rho R_1 = \max_{s_1} \left\{ b_1 - \frac{1}{2}c_0 s_1^2 + \lambda_0 s_1 \cdot \int_{\phi_1}^{\infty} \left(\frac{w}{\rho} - R_1 \right) dF(w) + I(s_1 < s^*) \cdot p_0 \cdot (R_2 - R_1) \right\} \quad (2)$$

$$\rho R_2 = \max_{s_2 | s_2 \geq s^*} \left\{ b_2 - \frac{1}{2}c_0 s_2^2 + \lambda_0 s_2 \cdot \int_{\phi_2}^{\infty} \left(\frac{w}{\rho} - R_2 \right) dF(w) \right\} \quad (3)$$

with $\rho R_1 = \phi_1$ and $\rho R_2 = \phi_2$

with $I(s_1 < s^*)$ being the indicator function of the event $s_1 < s^*$. Equations (2) and (3) can be understood by interpreting R_1 and R_2 as assets for which the return flow equals the flow of what one expects to get from holding the asset. In equation (2), the latter consists of three parts: (i) the flow of benefits net of search costs, (ii) the job offer arrival rate times the expected gain of finding a job over staying unemployed, and (iii) the rate at which a sanction arrives times the expected loss of such an event in comparison to when this event does not yet occur.

In the absence of the restriction that $s_2 \geq s^*$, the optimal search intensity s_2 after a sanction would follow from differentiation of (3) (see the references listed above). As a result,

$$s_2 = \max \left\{ s^*, \frac{\lambda_0}{c_0} \int_{\phi_2}^{\infty} \left(\frac{w}{\rho} - R_2 \right) dF(w) \right\} \quad (4)$$

In the absence of sanctions, the optimal search intensity s_1 would follow from differentiation of (2). Denote the solution by \hat{s}_1 , so

$$\hat{s}_1 = \frac{\lambda_0}{c_0} \int_{\phi_1}^{\infty} \left(\frac{w}{\rho} - R_1 \right) dF(w) \quad (5)$$

If sanctions are possible (i.e. if $p_0 > 0$) then the right-hand side of (2) is not continuous in s_1 at s^* , so then \hat{s}_1 is not necessarily optimal. If $\hat{s}_1 > s^*$ then $s_1 = \hat{s}_1$. Otherwise $s_1 = \hat{s}_1$ or $s_1 = s^*$, depending on which one gives the highest return.

Using obvious notation, the transition rates from unemployment to employment before and after imposition of a sanction equal

$$\theta_{u,1} = \lambda_0 s_1 \bar{F}(\phi_1) \quad (6)$$

$$\theta_{u,2} = \lambda_0 s_2 \bar{F}(\phi_2) \quad (7)$$

with $\bar{F} = 1 - F$.

The following results follow from the model.

Result 1. *At the moment at which a sanction is imposed, the transition rate from unemployment to employment jumps upward.*

To see this, first of all note that $R_2 < R_1$, for the reasons that $b_2 < b_1$ and the choice of search intensity after a sanction is restricted by $s_2 \geq s^*$. The fact that $R_2 < R_1$ implies that $\phi_2 < \phi_1$, so $\bar{F}(\phi_2) > \bar{F}(\phi_1)$. There also holds that $s_2 \geq s^*$, while $s_1 < s^*$ because otherwise a sanction could not have been imposed. This implies that $s_2 > s_1$. In sum, $\theta_{u,2} > \theta_{u,1}$.

Result 2. *The transition rate from unemployment to employment is smaller in a system without sanctions than in the system with sanctions, for all individuals who have a positive probability of getting a sanction in the latter system (including those who by chance do not have sanctions imposed during unemployment), and even for individuals who have a zero probability of getting a sanction in the latter system.*

To see this, consider an individual with $s_1 < s^*$ who has not yet been punished. If p_0 reduces to zero (i.e. if we examine the comparative statics when going to a system without sanctions), then R_1 increases, and, as a result, ϕ_1 increases. Because of this, s_1 decreases (note that $s_1 = \hat{s}_1 < s^*$, and \hat{s}_1 decreases). In sum, θ_1 decreases. Now consider individuals who have a zero probability of getting a sanction in the system with sanctions but who have $\hat{s} < s^* = s_1$. If p_0 reduces to zero then these individuals also increase their reservation wage and decrease their search intensity.

Result 3. *Both the transition rate from unemployment to employment and the rate at which a sanction arrives depend on all the variables that the individual uses to determine his strategy.*

To see this, note that $\theta_{u,1}$ and $\theta_{u,2}$ as well as $p(s)$ depend on s , which in turn depends on all the structural determinants.

The three results above are robust with respect to a number of assumptions that were made for expositional convenience, like the functional form assumptions on $\lambda(s)$, $c(s)$ and $p(s)$, the assumptions on events after imposition of a sanction, and the assumption that all structural determinants are constant as a function of unemployment duration. If the latter is relaxed then the transition rate from unemployment to employment as well as the rate at which a sanction arrives will

depend on the elapsed unemployment duration (i.e. they are duration dependent).

We now discuss some implications of the results above for empirical analysis. First of all, the effect of occurrence of a sanction on the individual transition rate from unemployment to employment is that this rate increases at the moment the sanction occurs. If individuals are homogeneous then the size of this effect can be estimated from an unemployment duration model in which the moment at which a sanction occurs is a (time-varying) exogenous covariate.

Note that the parameter λ_0 of the rate at which job offers are received depends on the selection and job offer behavior of employers. In particular, it increases in the probability that an employer offers a job to the individual given that an initial contact is made. The behavior of prospective employers will not be influenced by a sanction because they do not have any information on it. Now, if the individual search intensity is close to a physical maximum and the probability of job acceptance by the individual is almost equal to one, then the transition rate from unemployment to employment is mostly determined by the selection and job offer behavior of employers and by the technology of the matching process. In that case, there will hardly be any effect of a sanction on this transition rate.

According to Result 2, the fact that sanctions are possible affects the transition rate of *all* individuals (except for those who would have a very high search intensity anyway). This could be labeled the *ex ante* effect of the system with sanctions, as opposed to the *ex post* effect that occurs when a sanction is actually imposed. Because a reduced-form analysis of individual unemployment duration data does not provide an estimate of the *ex ante* effect, such an analysis cannot be used to evaluate the effect on unemployment duration of having a UI system with sanctions vis-à-vis a UI system without sanctions, even if one abstracts from macro issues. However, in general the transition rate is higher in a system with sanctions even if no sanctions are actually imposed on the individual. This implies that our results can be used to obtain a conservative estimate of the over-all effect of having a UI system with sanctions vis-à-vis a system without sanctions.

A third implication concerns empirical analysis in case of (unobserved) heterogeneity amongst individuals. Suppose individuals differ with respect to a characteristic that acts as a determinant of search behavior, and suppose that the individuals know their own value of this characteristic but that these values are not observed in the data we have. In that case both the transition rate from unemployment to employment θ_u and the rate at which a sanction arrives $p(s)$ depend on this unobserved characteristic. For example, suppose the non-pecuniary utility of being unemployed is high for one group of individuals and

low for another otherwise equivalent group. Then, for the first group, θ_u is small and $p(s)$ is large (say p_0) whereas for the second group θ_u is large and $p(s)$ is small (say zero). This creates a spurious relation between the duration until a sanction arrives and the duration of unemployment (this is the selectivity problem discussed in Section 1). Note that a similar spurious relation is created if the policy parameter p_0 of the sanction rate itself differs across individuals in a way that is not observed by the researcher.

We finish this subsection with the remark that a welfare-theoretic discussion on the optimality of different UI systems would be beyond the scope of the paper. In our model, the UI system is characterized by the values of the parameters $\{b_1, b_2, s^*, p_0\}$. If individuals are heterogeneous then it is conceivable that it is optimal to have a system in which s^* is binding for some fraction of the population. If s^* is very small, then nobody will ever get a sanction, the effect of the fact that sanctions are possible on the transition rates will at most be negligible, and the support for the social security framework may erode (so b_1 may have to be decreased).

2.2 A Multivariate Duration Model

In this subsection we present the multivariate duration model that we use in the empirical analysis. Before doing so, it is useful to outline the information we observe on the endogenous variables in the model (Section 3 gives a more complete description.) As noted in the introduction, the data contain all individuals who started collecting unemployment insurance (UI) benefits in 1992. Note that UI recipients are a subset of the unemployed. However, for a given individual, the date of inflow into UI as a rule coincides with the date of inflow into unemployment. For each individual we know the duration of being in UI, except when it is right-censored by the end of the observation period (late 1993). If the UI duration is completed then we know the exit state. This is usually either employment or "unemployment after completion of UI entitlement" (see Section 3). We do not have information on events occurring after leaving UI. Therefore, if the exit state is not employment then we regard the duration of going from unemployment to employment as being right-censored in the sense that all we know is that this duration exceeds the completed UI duration.

For all individuals we know whether or not they were punished with a sanction, and if so when that sanction started. Unfortunately we do not know the magnitude of the sanction or the length of the period during which benefits are reduced. Also, we only know the sanction date for the first sanction in a given

spell of unemployment. We return to these issues below.

The focal points in the empirical model are the transition rate from unemployment to employment and the rate at which the first sanction is imposed. Consider individuals receiving unemployment insurance benefits who are unemployed for t units of time. We assume that all individual differences in the transition rate from unemployment to employment at t can be characterized by observed characteristics x and unobserved characteristics v_u (with x and v_u independent in the inflow into unemployment), and by a variable indicating whether a sanction has already been imposed during the current unemployment spell. Let t_s denote the moment at which the first sanction is imposed on the individual during the current spell, and let I denote the indicator function which is 1 if its argument is true and 0 otherwise. At this stage, t_s itself is undefined if no sanction is imposed on the individual at all. In any case, the variable indicating whether a sanction has been imposed before t can now be expressed as $I(t_s < t)$.

For individuals receiving unemployment insurance benefits, the transition rate from unemployment to employment (or, to be short, the hazard) at t conditional on x, v_u and t_s is denoted by $\theta_u(t|x, v_u, t_s)$ and is assumed to have the familiar Mixed Proportional Hazard (MPH) specification,

$$\theta_u(t|x, v_u, t_s) = \lambda_u(t) \cdot \exp(x'\beta_u + \delta \cdot I(t_s < t) + v_u) \quad (8)$$

in which $\lambda_u(t)$ represents the individual duration dependence of the hazard.

Equation (8) summarizes a number of important assumptions. First, we assume that x is not time-varying. Second, we assume that a sanction does not affect the transition rate from unemployment to employment before the moment of imposition of the sanction. As explained in Subsection 2.1, it is unlikely that individuals anticipate the imposition of a sanction, or, at least, the exact date and magnitude of the sanction. Third, we do expect that a sanction, once imposed, has a permanent effect on the exit rate out of unemployment. This has also been motivated in Subsection 2.1. Fourth, we assume that the multiplicative effect of a sanction on the hazard is the same for every type of individual.

If detailed information were available on the length of the sanction period, the severity of the sanction, and the occurrence of multiple sanctions, then it would not be necessary to make all these assumptions. In that case it would be possible to estimate the behavior of the hazard shortly before, during, and after the sanction period, without the need to rely on functional form assumptions. Since our data do not contain this kind of information, we confine ourselves to the specification above. Later on we check the sensitivity of this specification by estimating models with alternative assumptions on the behavior of the hazard

close to the moment at which a sanction is imposed. We also test whether the parameter δ varies over x .

Let t_u denote the realized unemployment duration. The conditional unemployment duration density function $f_u(t_u|x, v_u, t_s)$ of $t_u|x, v_u, t_s$ over the inflow into unemployment can be written as

$$f_u(t_u|x, v_u, t_s) = \theta_u(t_u|x, v_u, t_s) \exp\left(-\int_0^{t_u} \theta_u(z|x, v_u, t_s) dz\right) \quad (9)$$

As argued in the introduction, we incorporate the distribution of the duration until imposition of a sanction into the model for the reason that the imposition of sanctions is not randomized. Consider the rate at which the first sanction is imposed on an individual receiving unemployment insurance, from the moment he enters the current spell of unemployment (the "sanction rate"). We assume that all individual differences in this rate can be characterized by observed characteristics x and unobserved characteristics v_s , with x and v_s independent in the inflow into unemployment. For an individual receiving unemployment insurance benefits who is unemployed for t units of time and who has not yet received a sanction, the sanction rate at t conditional on x and v_s is denoted by $\theta_s(t|x, v_s)$. This rate is assumed to have a MPH specification,

$$\theta_s(t|x, v_s) = \lambda_s(t) \cdot \exp(x'\beta_s + v_s) \quad (10)$$

in which $\lambda_s(t)$ represents the individual duration dependence of the hazard. For example, if, as suggested in the introduction, the rate at which a sanction is imposed increases just after entering unemployment, then $\lambda_s(t)$ increases for small t .

Note that t acts as time clock for both the unemployment duration and the duration until the first sanction. The variable t_s denotes the realization from the sanction duration distribution that is implicitly defined by (10). Note that t_s is a latent variable if $t_s > t_u$. The density of $t_s|x, v_s$ associated with the distribution defined by (10) can be expressed analogously to equation (9),

$$f_s(t_s|x, v_s) = \theta_s(t_s|x, v_s) \exp\left(-\int_0^{t_s} \theta_s(z|x, v_s) dz\right) \quad (11)$$

Now consider the joint distribution of t_u and t_s . Conditional on x, v_u and v_s , the only possible relation between the variables t_u and t_s is the relation by way of the direct effect of a sanction on the transition rate from unemployment to employment. This means that if $\delta = 0$ then, conditional on x , the variables t_u and t_s are only dependent if v_u and v_s are dependent. In case of independence

of v_u and v_s , we would have an ordinary duration model for t_u in which $I(t_s < t)$ can be treated as a time-varying regressor that is orthogonal to the unobserved heterogeneity term v_u .

However, if v_u and v_s are dependent, then inference on the distribution of $t_u|x, v_u, t_s$ has to be based on the joint distribution of $t_u, t_s|x$. For example, suppose v_u is positively related to v_s . Individuals who get a sanction early have on average larger v_s than individuals who get a sanction later. The former group of individuals therefore also have on average larger v_u than the latter group. This in turn implies they have on average larger transition rates from unemployment to employment than the latter group. Now if getting a sanction is assumed to be truly exogenous (i.e. independent of x and v_u), then the estimate of δ will be affected by this relation that works by way of the unobserved determinants. Specifically, δ will be over-estimated. It is also possible to have the reverse effect. For example, if lazy individuals have a lower hazard and a higher sanction rate, and if the model does not account for such a relation, then δ will be under-estimated.

The joint density $f_{u,s}(t_u, t_s|x)$ can be constructed as follows:

$$f_{u,s}(t_u, t_s|x) = \int_{v_u} \int_{v_s} f_u(t_u|x, v_u, t_s) f_s(t_s|x, v_s) dG(v_u, v_s) \quad (12)$$

in which G denotes the joint distribution of v_u, v_s in the inflow into unemployment, and f_u and f_s can be expressed in terms of the determinants of θ_u and θ_s . Because we observe versions of t_u, t_s (possibly multiple, possibly censored), and x , the individual likelihood contributions will be based on the density above (see Subsection 4.1).

As noted above, we may observe multiple spells of unemployment insurance, and therefore multiple durations until the imposition of a UI sanction, for a given individual. We assume that multiple $(t_u, t_s|x)$ combinations for one individual are statistically independent of each other. In addition, we assume that each combination is governed by the same probability laws and is affected by the same observed and unobserved explanatory variables x, v_u and v_s . This implies that the variables x, v_u and v_s are fixed across spells for a given individual. Some of these assumptions may be restrictive. For example, it may be that having had a sanction in a given unemployment spell affects behavior in subsequent unemployment spells. In Section 3 we examine these issues in a simple way.

2.3 Graphical checks

In this subsection we design a graphical method to obtain evidence on the presence of a sanction effect. This method also provides some clarification on the sources of identification in the empirical analysis. It should be noted from the outset that our method is fairly general and can be applied to other analyses of the effect of one random event on the other, in the presence of selectivity.

We start by examining the case in which only data with a single unemployment duration per individual are available. After that we examine what can be gained by using multiple spell data.

2.3.1 A check based on the timing of events in a single spell

At first sight it may seem impossible to obtain a simple graphical representation of a sanction effect. The most natural starting point would be to consider all individuals who are unemployed for t^* periods of time who have not yet received a sanction, and to compare those who do get a sanction at t^* to those who do not. This would consist of a comparison of the behavior of $\theta_u(t|t_u > t^*, x, t_s = t^*)$ to the behavior of $\theta_u(t|t_u > t^*, x, t_s > t^*)$, on $t > t^*$. However, it is clear that any such comparison is affected by the fact that, by conditioning on t_s , one examines selective subsets of individuals. Any observed difference may be due to correlated unobserved heterogeneity.

One may argue that a way to get around this is to condition on t_s and compare what happens before and after t_s . So consider the subset of individuals who are unemployed for t^* periods of time, who have not been given a sanction before t^* , and who do get a sanction at t^* , so $t_u > t^*$ and $t_s = t^*$ (note that a sanction can only be logically observed if the unemployment duration has not yet been completed). According to the model, the transition rate from unemployment to employment $\theta_u(t|x, v_u, t_s = t^*)$ jumps at $t = t^*$ if $\delta \neq 0$. This can also be shown to be true for the transition rate $\theta_u(t|x, t_s = t^*)$. (Of course, these transition rates may also jump at $t = t^*$ for other reasons, e.g. if the baseline hazard $\lambda_u(t)$ jumps at $t = t^*$; see below.) However, it is not possible to *observe* a jump of $\theta_u(t|x, t_s = t^*)$ at $t = t^*$, for the simple reason that if we condition on $t_u > t^*$ then there are no exits out of unemployment before t^* , so $\theta_u(t|x, t_s = t^*)$ is unobserved before $t = t^*$.

We now propose our own graphical method. Basically, the trick we use is to condition on t_u and examine what happens before t_u , rather than condition on t_s and examine what happens after t_s . Consider the subset of individuals who leave unemployment at a given duration t_u . Note that t_u is always observed, whether

$t_s < t_u$ or not, so any t_u can be chosen here. The intuition of our result is as follows. If a sanction increases the transition rate rate from unemployment to employment (i.e. if $\delta > 0$) then a relatively large fraction of those who make this transition at t_u have been given a sanction shortly before t_u . Thus, conditional on t_u , the rate at which a sanction is given $\theta_s(t|x, t_u)$ will tend to increase shortly before $t = t_u$.

Consider the simple model in which there is no duration dependence or unobserved heterogeneity. The latter assumptions allow us to write $\theta_u(t|x, v_u, t_s)$ as $\theta_{u,1} \cdot \delta_0^{I(t_s < t)}$, with $\theta_{u,1} := \lambda_u(t) \cdot \exp(x'\beta_u + v_u)$ and $\delta_0 := e^\delta$. Also, we can write $\theta_s(t|x, v_s)$ as θ_s , with $\theta_s := \lambda_s(t) \cdot \exp(x'\beta_s + v_s)$. Note that $\delta > 0$ iff $\delta_0 > 1$. It is also useful to define

$$\delta_0^* := \theta_s + (1 - \delta_0)\theta_{u,1} \quad (13)$$

This notation is only used in the present subsection.

After some tedious calculations we obtain

$$\theta_s(t|x, t_u) = \frac{\delta_0^* \delta_0 \theta_s}{\delta_0 \theta_s + (1 - \delta_0)(\theta_{u,1} + \theta_s) e^{(-\delta_0^*(t_u - t))}} \quad \text{with } t \in [0, t_u] \quad (14)$$

for the generic case in which $\delta_0^* \neq 0$. Note that numerator and denominator in the right-hand side of equation (14) have the same sign as δ_0^* . For the special case in which $\delta_0^* = 0$ (which implies that $\delta > 0$) we obtain

$$\theta_s(t|x, t_u) = \frac{\delta_0 \theta_s}{1 + \delta_0 \theta_s (t_u - t)} \quad \text{with } t \in [0, t_u] \quad (15)$$

The main implication of this is that

$$\theta_s(t|x, t_u) \text{ increases in } t \iff \delta > 0 \quad (16)$$

This is true for all parameter values and for any t_u , and, given t_u , for any $t \in [0, t_u]$. Moreover, it can be shown that if $\delta > 0$ then $\theta_s(t|x, t_u)$ is convex in t on $(0, t_u)$. Note that in any case $\theta_s(t|x, t_u)$ only depends on the difference of t_u and t .

The result in the simple model above shows that the realized value of t_u conveys a very specific kind of information on the moment at which a sanction is applied. If $\delta > 0$ then, given any realization of t_u , the rate at which a sanction is given will tend to increase shortly before t_u .

Now let us examine what happens if we allow for duration dependence, i.e. if λ_u and λ_s are allowed to depend on the corresponding elapsed duration. In that case the shape of $\theta_s(t|x, t_u)$ as a function of t will reflect this. For example, if $\lambda_s(t)$ displays a peak at a certain value of t then $\theta_s(t|x, t_u)$ also displays such a peak. This is true for any given value of t_u . In addition, it can be shown that the main feature of the earlier result is still valid. That is, if $\delta > 0$ then $\theta_s(t|x, t_u)$ tends to increase when t is just below t_u , in comparison to the shape of $\theta_s(t|x, t_u)$ at these t that prevails if one conditions on a larger t_u . This holds for all parameter values and for any t_u . So the way in which $\theta_s(t|x, t_u)$ varies with t reflects duration dependence, while the way in which $\theta_s(t|x, t_u)$ varies with $t - t_u$ reflects the sanction effect. This means that a valid graphical way to detect the sanction effect is to plot $\theta_s(t|x, t_u)$ as a function of t for different given values of t_u .

To put this differently, $\theta_s(t|x, t_u)$ may decrease shortly before a given t_u , but this may be due to duration dependence, and this can be checked for by comparing the graph to the graphs that correspond to larger values of t_u . If $\delta > 0$ then, shortly before t_u , $\theta_s(t|x, t_u)$ should diverge in the upward direction from the graphs that correspond to larger values of t_u .

Now let us examine what happens if we allow for unobserved heterogeneity. In that case the value of t_u on which we condition is informative on the unobserved determinant v_s of t_s . The main effect of this is that the mean value of $\theta_s(t|x, t_u)$ may strongly depend on t_u . Obviously, this effect does not affect the validity of the graphical check above. However, we cannot completely rule out that the unobserved heterogeneity distribution is such that it generates peaks of $\theta_s(t|x, t_u)$ shortly before t_u for some different values of t_u , such that it mimics the picture that prevails in case there is no heterogeneity but $\delta > 0$. While this would require a very specific kind of heterogeneity distribution, it does suggest that some caution should be taken, and that the number of different t_u values should not be too small.

In Subsection 3.2 we apply the method above to obtain evidence on the presence of a sanction effect.

2.3.2 Checks based on multiple spells

Our data contain multiple unemployment spells for some individuals. Intuitively it may seem that these can be used in a simple way to obtain an indication of a sanction effect. However, some caution has to be taken here. Suppose we observe the exact durations of two spells for each individual. Consider those individuals who get a sanction in one of those spells but not in the other. The sanction

effect is positive if the mean duration of the spells within which a sanction is imposed is smaller than the mean duration of the other spells. However, the converse is not necessarily true, simply because it takes time to get a sanction. For example, if every individual will get a sanction at a duration of say 6 months, then the mean duration of the spells within which a sanction is imposed exceeds the mean duration of the other spells, even if there is a positive sanction effect. This problem can be circumvented by restricting attention to spells with a certain minimum duration (say 3 months). In that case, spells in which a sanction is imposed somewhere in the first three months are to be compared to spells in which this does not happen, for the same individuals.

Unfortunately, this approach is not feasible either, in the present context. There is no information on labor market behavior after September 1993, and, as a result, all spells that were ongoing at that time have right-censored durations. The first implication of this is that we do not know whether a sanction is imposed after September 30, 1993, in spells that are ongoing at that date, and that we do not know the corresponding realized duration. This problem can be circumvented by restricting attention to spells that start before July 1, 1993. Consider for ease of exposition individuals who get such a sanction in their first spell but not in their second spell. As such, the right-censoring at September 30 is not a problem for the estimation of (characteristics of) the distribution of the “first” duration. However, it is a problem if one wants to estimate (characteristics of) the distribution of “second” duration in isolation from the estimation of the “first” duration. This is because the moment at which the second duration is right-censored is not independent from the duration itself. To see this, consider individuals for which v_u is large. For these individuals, the first duration t_{u1} will on average be short. We assume that the subsequent employment spell is i.i.d. across individuals and is not related to v_u and v_s . As a result, the second unemployment spell will on average start at a relatively early moment. This in turn implies that the second spell will often be right-censored at a relatively long duration. In sum, the second duration t_{u2} and the variable determining the moment at which it is censored are both affected by the unobserved characteristic v_u . This violates the standard “independent right-censoring” assumption of duration analysis (there is an analogy to the analysis of informative attrition in panel survey data; see Van den Berg, Lindeboom and Ridder (1994)). The general idea that right-censoring of a “second” duration may not be independent if the first and second durations are dependent was established by Visser (1996).

One way to get around this problem is to consider even smaller subsets of the data. For example, one may select all individuals who enter unemployment

in January 1992, leave unemployment after 3 months without having received a sanction, and re-enter unemployment in January 1993, in order to check whether those who do receive a sanction in the first three months of their second unemployment spell on average have a second duration of less than three months. However, as will be shown in Section 3, such subsamples would be much too small for a meaningful analysis. In sum, the data on multiple spells cannot be used in a simple way to obtain an indication of a sanction effect.

2.3.3 Some remarks on identification

A complete formal proof of the nonparametric identification of the model is beyond the scope of the paper. However, the results above provide some clarification on the sources of identification in the empirical analysis. First of all, it is clear from the graphical method proposed above that the consecutive timing of sanctions and exits out of unemployment in single spell unemployment duration data is very informative on the magnitude of the sanction effect parameter δ . Secondly, in an ideal situation (no right-censoring), multiple spell data are also very informative on this, but, as will be shown in Section 3, our data are not sufficiently rich to exploit this.

It is well known that the MPH duration model is identified from single spell duration data (for a survey, see Lancaster (1990)). However, identification depends crucially on the assumptions underlying the MPH framework. Also, it is generally believed that in practice estimation is next to impossible without (semi-)parametric assumptions, and that the results may depend heavily on these assumptions. These issues may be even more relevant for bivariate duration models in which one of the durations is included in the model in order to deal with a selection effect for the other duration. Multiple spell data enable identification under weaker assumptions, in particular if the unobserved heterogeneity term is fixed across spells (Honoré (1993), Heckman and Taber (1994)).

We illustrate the advantages of multiple spells by focusing on the identification of the baseline hazard λ_u of the transition rate from unemployment to employment. Let $\Theta_u(t|x, v_u, t_s)$ denote the integrated hazard associated with $\theta_u(t|x, v_u, t_s)$, i.e.

$$\Theta_u(t|x, v_u, t_s) = \int_0^t \theta_u(z|x, v_u, t_s) dz \quad (17)$$

It is well known that $\Theta_u(t_u|x, v_u, t_s)$ given x, v_u and t_s has an exponential distribution with mean one (see e.g. Lancaster (1990)). Now suppose we observe two unemployment durations t_{u1} and t_{u2} for all individuals. Given x and v_u

and given the (possibly latent) sanction durations t_{s1} and t_{s2} , the difference $\Theta_u(t_{u1}|x, v_u, t_{s1}) - \Theta_u(t_{u2}|x, v_u, t_{s2})$ is distributed as the difference of two independent random variables each having an exponential distribution with mean one. By elaborating on the difference of the integrated hazards, it follows that, given t_{s1} and t_{s2} ,

$$\begin{aligned} & \text{I}(t_{u1} < t_{s1}) \cdot \log \Lambda_u(t_{u1}) + \text{I}(t_{u1} > t_{s1}) \cdot \log [\Lambda_u(t_{s1}) + e^\delta (\Lambda_u(t_{u1}) - \Lambda_u(t_{s1}))] \\ & - \text{I}(t_{u2} < t_{s2}) \cdot \log \Lambda_u(t_{u2}) + \text{I}(t_{u2} > t_{s2}) \cdot \log [\Lambda_u(t_{s2}) + e^\delta (\Lambda_u(t_{u2}) - \Lambda_u(t_{s2}))] \end{aligned} \quad (18)$$

is distributed as the difference of two independent random variables each having an exponential distribution with mean one. In this equation, $\Lambda_u(t)$ denotes the integrated baseline hazard associated with $\theta_u(t|x, v_u, t_s)$, i.e.

$$\Lambda_u(t) = \int_0^t \lambda_u(z) dz \quad (19)$$

Note that the unobserved heterogeneity term v_u cancels out of the difference in (18). Given t_{s1} and t_{s2} , and given a value of δ , this difference is informative on the shape of Λ_u , i.e. on the duration dependence of θ_u .

2.4 Parameterization

For the duration dependence functions and the bivariate unobserved heterogeneity distribution we take the most flexible specifications used to date. We take both $\lambda_u(t)$ and $\lambda_s(t)$ to have a piecewise constant specification,

$$\lambda_i(t) = \exp \left(\sum_j \lambda_{ij} I_j(t) \right) \quad i = u, s \quad (20)$$

where j is a subscript for time intervals and $I_j(t)$ are time-varying dummy variables that are one in consecutive time intervals. Note that with an increasing number of time intervals any duration dependence pattern can be approximated arbitrarily closely. By now it is well known that duration dependence specifications with only one parameter (like a Weibull specification) are overly restrictive and may cause the estimated unobserved heterogeneity distribution to be biased (see e.g. Lancaster (1990)).

In the empirical literature on univariate labor market durations, unobserved heterogeneity is often modeled by way of a discrete random variable (see e.g.

Nickell (1979) and Ham and Rea (1987)). Usually, if more than two or three points of support are taken then the estimates of some of them coincide. Heckman and Singer (1984) show that in MPH models the non-parametric maximum likelihood estimator of the heterogeneity distribution is a discrete distribution. However, the estimation procedure requires the number of points of support not to be fixed in advance, and estimation of standard errors is not straightforward. Moreover, the procedure does not deal with endogenous time-varying covariates. Nevertheless, this result illustrates the flexibility of discrete distributions as heterogeneity (or mixture) distributions.

In most applied papers in which multiple duration variables depend on each other by way of their unobserved determinants, a one-factor loading specification is used for the multivariate heterogeneity distribution. This means that the log heterogeneity terms are assumed to be linear functions of a single random variable ω , so e.g. $v_i = \exp(c_{i0} + c_{i1} \cdot \omega)$. This restricts the way that the v_i are related. Lindeboom and Van den Berg (1994) show that in such models there may not be enough flexibility in order to obtain correct estimates of the variances of the duration variables as well as of their interrelation. A genuine multivariate specification for the heterogeneity distribution is to be preferred.

Van den Berg (1996) examines the range of values that the correlation of the duration variables can attain in multivariate MPH models (with constant and exogenous covariates), in general as well as for particular parametric families of the multivariate heterogeneity distribution. It turns out that when the heterogeneity terms have a multivariate discrete distribution with two or more points of support for each, and the locations of these points are not fixed in advance, then all possible correlation values can be attained. On the other hand, when e.g. the log heterogeneity terms have a multivariate normal distribution, or when they have a multivariate discrete distribution in which the locations of the points of support are fixed in advance, then the range of values that can be attained is smaller.

Taken together, these results suggest that in the present context, multivariate discrete heterogeneity distributions with unrestricted mass point locations provide maximum flexibility. This is the approach we will follow here. (See Coleman (1990), Van den Berg, Lindeboom and Ridder (1994) and Van den Berg and Lindeboom (1994) for other examples of the use of multivariate discrete heterogeneity distributions with unrestricted mass point locations.) Note that discrete distributions are also attractive from a computational point of view.

We take v_u and v_s to have two points of support each (v_u^a, v_u^b, v_s^a and v_s^b , respectively). The associated probabilities are denoted as follows:

$$\begin{aligned}
\Pr(v_u = v_u^a, v_s = v_s^a) = p_1 & & \Pr(v_u = v_u^b, v_s = v_s^a) = p_2 \\
\Pr(v_u = v_u^a, v_s = v_s^b) = p_3 & & \Pr(v_u = v_u^b, v_s = v_s^b) = p_4
\end{aligned} \tag{21}$$

with $0 \leq p_i \leq 1$ for $i = 1, \dots, 4$, and $p_4 = 1 - p_1 - p_2 - p_3$.

The covariance of v_u and v_s equals

$$\text{cov}(v_u, v_s) = (p_1 p_4 - p_2 p_3) \cdot (v_u^a - v_u^b) \cdot (v_s^a - v_s^b) \tag{22}$$

It is easy to show that v_u and v_s are independent if and only if $\text{cov}(v_u, v_s) = 0$. Now suppose $v_u^a \neq v_u^b$ and $v_s^a \neq v_s^b$ (i.e. suppose there is dispersed heterogeneity). Then the correlation of v_u and v_s does not depend on the magnitudes of v_u^a, v_u^b, v_s^a and v_s^b . Further, the variables v_u and v_s are perfectly correlated if $p_1 = p_4 = 0$ or $p_2 = p_3 = 0$.

3 Benefits and sanctions in the Netherlands

3.1 Institutional aspects

The aim of the Unemployment Law in the Netherlands is to insure employees against the financial consequences of unemployment. The current law dates from January 1, 1987. It is not our intention to give a full and detailed description of this law. Instead we highlight the relevant aspects of the law to give a flavor of its basic structure, sufficient to illustrate the way we model the different processes involved.

If a worker loses his job in the Netherlands then he is entitled to unemployment benefits, provided some conditions are fulfilled. The unemployed has to face a reduction of half of his original working hours with a minimum of five, he should not get paid for this working hour reduction and he should be willing to accept a new job. Furthermore, unemployed should have a job history in which they have had a job for at least 26 weeks in the past 52 weeks prior to the start of the unemployment period. Unemployed who fulfill these conditions are entitled to two subsequent unemployment insurance benefits: the initial benefits and the extended benefits. The initial benefits are equal to 70% of the wage in the last job before unemployment. The maximum duration of these benefits ranges from 6 months to 4.5 years. The exact duration depends on the employment history of the unemployed. For example, to get an initial benefits entitlement period of 4.5

years, the unemployed worker has to have had jobs for a total period of 40 years. The extended benefits are equal to 70% of the minimum wage or 70% of the wage in the last job before unemployment whichever is less. The maximum duration of the extended benefits is one year. If after the expiration of the extended benefits the unemployed has not found a job, he may receive subsistence benefits (social assistance) which is related to his household income and to what is considered to be the social minimum benefits.

According to the Unemployment Law, an unemployed worker has several obligations in order to be entitled to collect unemployment insurance benefits: he has to (i) prevent unnecessary job loss, (ii) take actions to prevent him from staying unemployed, so he has to search for a job and accept job offers, register as a job searcher at the public employment office, participate in education and training, etcetera, and (iii) keep the administrative organization informed about everything that is relevant to the payment of the unemployment insurance benefits.

The administration of the unemployment benefits system is organized at the level of the industry. If an unemployed worker does not live up to the rules then the administrative organization is authorized (not obliged) to apply a sanction to that worker. This sanction may be applied as a temporary or permanent full or partial reduction of the benefit. In practice the temporary partial reduction of the benefits ranges from 5% during 4 weeks to 25 or 30% during 13 weeks. In a description of a sample of UI recipients, Besseling and Aan de Kerk (1994) find that, of all sanctions that are given in the first 7 months of unemployment (including those given at the start of unemployment), about half entails a benefits reduction of 5%-15%, while about the other half entails a reduction of 15%-35%. It is also possible that the maximum duration of the unemployment benefits is reduced.

Some offenses are more easy to detect than others. At the start of his unemployment period the worker has to give the UI administration agency all kinds of information about his previous job and the way he got unemployed. Therefore, it is quite easy to establish whether or not the worker is to blame because of insufficient action to prevent job loss. It is much more difficult to establish whether or not the worker is to blame because of insufficient effort to find a job. Because of this, a large part of the actual UI sanctions are those because of lack of action to prevent job loss. For example, in 1992, 29,000 out of a total of 76,000 sanctions were given because of this type of offence. In the aforementioned study of Besseling and Aan de Kerk (1994), less than 10% of the sanctions given after the start of unemployment and before 7 months concerns a sanction given because of

work on the “black labor market”. More than 90% concerns the search-related reasons mentioned above.

The Unemployment Law does not specify which type of sanction has to be applied to which offence. So the UI administration agencies are free to choose the sanctions which they think are appropriate. As a consequence of this, the application of UI sanctions depends on a large number of arguments some of which are left to the discretion of the employees of the UI administration agency. Whether or not a sanction is applied and if so which sanction is applied will depend on the nature of the offence, the labor market situation in the specific industry, the personal and family situation of the unemployed worker involved, etcetera.

3.2 The Data

Our database originates from the Dutch Social Security Council and consists of unemployed who lost their job in the metal industry and the banking sector and who applied for and started collecting unemployment benefits in 1992. The information about the unemployment benefits durations and sanctions is collected up to September 1993. There is no information on individuals after they stop collecting UI benefits, but we know whether they stop because they find a job or because the period during which they can claim unemployment benefits comes to an end. (The Appendix contains a very extensive description of the data.)

Observations of durations are classified in intervals, so we know whether an individual received unemployment benefits during a specific interval and whether a sanction was imposed during that same interval. Some of the unemployed were still collecting benefits in September 1993. Thus, these observations are right-censored. There are also right-censored observations if UI stopped for a different reason than transition to employment. One of these reasons may be that the unemployed received the benefits for his maximum entitlement period.

There is information on the moments sanctions are imposed, but we have no information on the magnitude of sanctions. We only use information about sanctions that were imposed during the spell of unemployment, omitting spells with sanctions that were imposed at the start of the unemployment spell. The reason for this is that it is not possible to identify the selectivity involved in imposition of sanctions at the start of the spell. Also, recall that sanctions at the start are given for reasons related to behavior before unemployment, which are very different from the reasons for sanctions during the spell.

In some cases we observe multiple spells for a single individual. As mentioned

before this information can be exploited to improve the efficiency of the analysis.

As indicated, we use information from two samples corresponding to two sectors of the economy (and corresponding to two different UI agencies), one of which is an industrial sector and one of which is a service sector. The Metal Industry sample contains about 7,000 individuals; the sample of the Banking sector contains about 26,000 individuals. Table 1 provides some statistics for both samples. The inflow of unemployed that came from a job in the metal industry has characteristics that differ from the inflow of unemployed coming from the banking sector. The main differences concern sex, region, urbanization of the geographical area and whether the unemployed had a part time or a full time job. The inflow into unemployment coming from the metal industry is predominantly male, while the inflow from the banking sector has about the same number of males and females. There is also a difference with respect to the region. For the metal industry about one third of total inflow into unemployment is located in the south of the Netherlands, while for the banking sector half of the inflow into unemployment is in the western part of the Netherlands. It also appears that whereas for the metal industry 10% of the inflow is in highly urbanized areas, this is 20% of the inflow for the banking sector. A similar difference concerns the inflow of unemployed that have had part time jobs. For the metal industry this is 10%, for the banking sector it is 20%. The differences with respect to other characteristics like wages on the previous job and the marital status are only minor. There is also only a minor difference with respect to the percentage of unemployed who were reduced during their spell of unemployment. This is 2.1% for the metal industry and 2.4% for the banking sector.

As mentioned before, the number of explanatory variables is limited due to the administrative character of our data set. This has consequences for the possibility of constructing natural experiments to deal with the selectivity problem. One could be tempted to exploit the differences in implementation of the sanction instrument between the UI agencies of different sectors in order to identify the sanction effect. Certain occupations (like secretary) are present in different sectors and therefore are tied to different UI agencies with different sanction policies. However, we cannot exploit this, simply because we do not observe the occupation of the individuals in the sample. Other variables that could be useful from this point of view, like education, are also missing.

The data do not contain the exact magnitude of the individual UI benefits level. However, this is a monotone function of the wage earned before entering unemployment, affected by personal and household characteristics. The wage as well as these characteristics are observed. Note that the wage is also correlated

to the productivity of the individual.

In general, the data do not provide information on the individual maximum UI entitlement, except of course when the individual is observed to complete entitlement. This is unfortunate to the extent that this variable is an important determinant of $\theta_u(t|x, v_u, t_s)$ and $\theta_s(t|x, v_s)$. On the one hand, it is a well established fact that the exit rate out of unemployment tends to increase shortly before the maximum entitlement date (Meyer (1990), Van den Berg (1990a)). On the other hand, it is conceivable that the optimal search intensity shortly before expiration is such that the rate of getting a sanction is higher than otherwise (for example, the individual may not be entitled to any subsequent benefits, so that benefits will drop to zero anyway).

Figure 1 displays the graphical check that was designed in Subsection 2.3.1. To obtain a sensible graph (i.e., to obtain a sufficiently large number of spells), we use data from the whole population, including all sectors of the economy. It is clear that the graph provides strong evidence for the presence of a positive sanction effect on the individual transition rate into employment. This result will be confirmed below.

4 Estimation of the model

In this section we discuss estimation of our model. First, we derive the likelihood of the empirical model specified in Section 2. Next, we present and discuss the parameter estimates. Then, we investigate the sensitivity of the estimation results with respect to the use of multiple spells, the specification of the unobserved heterogeneity distribution and the sanction effect and the possibility of a duration dependent sanction effect. Discussion of simulations with the estimated model is postponed to Section 5.

4.1 The likelihood function

The individuals in the data we use are sampled from the total inflow into unemployment insurance in 1992. Thus, the joint durations t_u and t_s constitute an inflow sample without initial conditions problems.

We first derive the likelihood contributions of observations of single spells, conditional on the observed and unobserved characteristics. We have to consider both right-censoring and interval-censoring of the durations. Here, we use “interval-censoring” only for situations in which it is only known that the value of the variable under consideration is in a *finite* interval. The interval-censoring

is a consequence of the data design (see Subsection 3.2): events are only observed to occur in fixed intervals for the elapsed durations.

Four cases can be distinguished: I exit into employment is observed to occur before a sanction has been imposed, II both the unemployment duration and the duration until imposition of a sanction are right-censored at the same value, III imposition of a sanction is observed to occur in a time interval before the interval within which exit out of unemployment occurs, and IV imposition of a sanction and exit out of unemployment are observed to occur in the same time interval. In Case I, t_u is interval-censored and t_s is right-censored. In Case II, both t_u and t_s are right-censored. In Case III, t_s is interval-censored and t_u is interval-censored or right-censored. Finally, in Case IV, both t_u and t_s are interval-censored.

It is useful to define the following functions.

$$S_{u,1}(t|x, v_u) = \exp\left(-\int_0^t \lambda_u(z) \exp(x'\beta_u + v_u) dz\right) \quad (23)$$

$$S_{u,2}(t|x, v_u, t_s) = S_{u,1}(t_s|x, v_u) \cdot \exp\left(-\int_{t_s}^t \lambda_u(z) \exp(x'\beta_u + \delta + v_u) dz\right) \quad (24)$$

$$S_s(t|x, v_s) = \exp\left(-\int_0^t \lambda_s(z) \exp(x'\beta_s + v_s) dz\right) \quad (25)$$

$S_{u,1}$ on $[0, t_s)$ and $S_{u,2}$ on $[t_s, \infty)$ constitute the survivor function associated with $t_u|x, v_u, t_s$. Similarly, S_s constitutes the survivor function associated with $t_s|x, v_s$.

Consider Case I, i.e., consider a spell within which no sanction is imposed, and which is observed to end within the interval $(\underline{T}_u, \overline{T}_u)$ for t_u . Conditional on the observed and unobserved characteristics, the probability of these events equals

$$\int_{\underline{T}_u}^{\overline{T}_u} \lambda_u(t) \exp(x'\beta_u + v_u) S_{u,1}(t|x, v_u) S_s(t|x, v_s) dt \quad (26)$$

In Case II, the conditional likelihood contribution of a spell that is right-censored at T equals

$$S_{u,1}(T|x, v_u) S_s(T|x, v_s) \quad (27)$$

In Case III, consider a spell for which t_s is realized within $(\underline{T}_s, \overline{T}_s)$ and t_u is realized within $(\underline{T}_u, \overline{T}_u)$, with $\overline{T}_s \leq \underline{T}_u$. The conditional likelihood contribution of this spell equals

$$\int_{\underline{T}_s}^{\overline{T}_s} \lambda_s(t_s) \exp(x'\beta_s + v_s) S_s(t_s|x, v_s) \cdot [S_{u,2}(\underline{T}_u|x, v_u, t_s) - S_{u,2}(\overline{T}_u|x, v_u, t_s)] dt_s \quad (28)$$

In Case IV, consider a spell for which both t_u and t_s are observed to lie within the interval $(\underline{T}_s, \bar{T}_s)$ (note that this implies that $t_s \leq t_u$). The conditional likelihood contribution of this spell equals

$$\int_{\underline{T}_s}^{\bar{T}_s} \lambda_s(t_s) \exp(x' \beta_s + v_s) S_s(t_s | x, v_s) \cdot [S_{u,2}(t_s | x, v_u, t_s) - S_{u,2}(\bar{T}_s | x, v_u, t_s)] dt_s \quad (29)$$

It is trivial to modify the expression above for Case III if t_u is right-censored at the end of the period of observation. For reasons of brevity we do not spell out the likelihood contributions in case t_u is right-censored due to an exit into non-participation. Note that, because of equation (20), all integrals in all conditional likelihood contributions can be solved explicitly. Also note that all contributions only depend on the hazard rates for durations smaller than the maximum period of observation.

Next, we derive the unconditional likelihood of an observation of a set of unemployment spells of a single individual. Say we have N individuals, numbered $1, 2, \dots, N$. Suppose individual $n \in \{1, 2, \dots, N\}$ experiences $M_n \in \{1, 2, \dots\}$ unemployment spells. Denote the conditional likelihood contribution of spell $m \in \{1, \dots, M_n\}$ of this individual by $\mathcal{L}_{mn}(v_u, v_s)$ (we suppress conditioning in x). Then, the unconditional likelihood of the observations of this individual's spells is given by

$$\mathcal{L}_n = \sum_{v_u, v_s} \left[\prod_{m=1}^{M_n} \mathcal{L}_{mn}(v_u, v_s) \right] dG(v_u, v_s). \quad (30)$$

As individual observations are supposed to be independent, the log likelihood is given by

$$\log \mathcal{L} = \sum_{n=1}^N \log \mathcal{L}_n. \quad (31)$$

We specify all 11 covariates in deviation of their sample means. Age is divided by 10, wages by 100. The squares of age and wages in deviation from their sample means are included as regressors. Furthermore, we specify 7 duration intervals within the piecewise constant duration dependence functions are assumed to be constant. In particular, we take (in weeks) $I_0 = [0, 8), I_1 = [8, 16), I_2 = [16, 24), I_3 = [24, 32), I_4 = [32, 45), I_5 = [45, 58)$ and $I_6 = [58, \infty)$. Thus, we are left with estimating parameters $v_u^a, v_u^b, v_s^a, v_s^b, \delta, \beta_{u,i}$ and $\beta_{s,i}, i = 1, \dots, 11, \lambda_{u,t}$ and $\lambda_{s,t}, t = 0, \dots, 6, p_1, p_2,$ and p_3 . We normalize by taking $\lambda_{u,0} = \lambda_{s,0} = 0$.

4.2 Estimation results

We estimate the model using samples from the the metal industry and banking sector data sets. First, we estimated the full model described in Section 2. From these estimates a simple pattern of duration dependence of the sanction hazards emerges: the hazard rate is low in the first 8 weeks and higher, but constant, in the weeks thereafter. Furthermore, the pattern of unobserved heterogeneity for the hazard rates of the banking sector shows perfect correlation between the unobserved heterogeneity components of both hazard rates. Therefore, we impose restrictions on the full model accordingly ($\lambda_{s,1} = \lambda_{s,2} = \dots = \lambda_{s,6}$ for both samples, and $p_2 = p_3 = 0$ for banks). Likelihood Ratio (LR) tests confirm that estimates of the restricted models are not significantly different from those of the full model. The LR statistics for the restrictions on the full model are distributed χ^2_5 and χ^2_7 , and equal 5.74 and 4.84 for the metal industry and the banking sector, respectively.

Tables 2 and 3 present the parameter estimates for both restricted models. The first columns contain estimated values of the parameters of the re-employment hazard rate, the second columns estimates of the parameters of the sanction hazard rate. Asymptotic standard errors are included in parentheses. A heterogeneity mass point parameter of $-\infty$ indicates that the likelihood attains a maximum at a boundary of the parameter space. Thus, we estimated this model with the relevant conditional hazard rates fixed at 0.

The central parameter is the effect parameter δ . The estimate of δ does not differ a lot across the two sectors, it being 0.57 for the metal industry and 0.81 for the banking sector. The estimate of δ is significantly larger than 0 for both the metal industry and the banking sector. Clearly, sanctions raise the transition rate from unemployment to employment. Indeed, for all means and purposes, the effect is considerable. For the metal (banking) sector, the individual transition rate increases by 80% (120%) when a sanction is given. Computations that shed more light on the magnitude of this positive effect are presented in Section 5.

A second issue is the effect of observed heterogeneity on both hazard rates. It appears that both sanction hazard rates of the metal industry and the banking sector are influenced in the same way by most of the variables. We find negative effects for age, wage and marital status. For the metal industry we find a significant positive coefficient for the south, which we do not find for the banking sector. For the re-employment hazard rate we also find very similar effects for the metal industry and the banking sector. We find that age has a negative effect, while wage has a positive effect on the re-employment hazard. Furthermore, females and married unemployed have a lower hazard rate to a job than males

and unmarried unemployed. Finally, unemployed in highly urbanized areas and in the north have a lower than average probability to find a job.

A third issue of interest is the selectivity of imposition of sanctions. From the two estimation tables we can infer that the imposition of sanctions is indeed selective. Both unobserved and observed heterogeneity appears to induce dependencies between the hazard rates for transition to employment and imposition of sanctions. Table 2, for instance, shows that in the metal industry married unemployed workers are both significantly less likely to re-enter employment and significantly less likely to having a sanction imposed. Omitting marital status as an explanatory variable would have resulted in overestimation of δ . Similarly, the negative correlation between v_u and v_s indicates that analysis with a model without correlated unobserved characteristics would have produced a downward bias in the estimate of δ . Various other examples can be found in both tables. The main message is that correcting for selectivity is necessary in order to get unbiased estimates of the effect of sanctions.

In general one could say that younger people both face higher sanction rates and have higher transition rates to employment. Urban people have lower exit rates to employment, but higher sanction rates in both the metal industry and the banking sector. Implications for the other observed characteristics vary over the two samples and are again clarified below where we present results of simulations.

The estimates of parameters of the unobserved heterogeneity distribution are generally consistent with significant effects of unobserved heterogeneity. For the metal industry this leads to positive correlation between the two hazard rates. However, the banking sector estimates imply negative correlation. Salient characteristic of the estimated heterogeneity distribution for this subsample is that one of the support points of v_u converges to its boundary, $-\infty$. So, if we believe in a simple extrapolation beyond the maximum duration actually observed, we may conclude that some individuals will, for a reason not observed by us, never find a job.

The fourth issue is duration dependence of the hazard rates. In both sectors exit rates to employment drop when the spell of unemployment continues. Only after 45 weeks these exit rates stop declining. Apparently, issues like stigmatization and discouragement play a significant role in individual unemployment durations. Furthermore, the hazard rate of sanction imposition rises significantly after 8 weeks. This is consistent with the fact that the sanctions we consider are sanctions that are imposed because of some violation of the unemployment insurance regulation by the benefits recipient during the unemployment spell. Clearly, such a violation during the unemployment spell cannot lead to a sanction right

at the start of that same spell.

4.3 Sensitivity analysis

In this subsection we discuss sensitivity analyses, specification tests, and results for extended model versions. We focus in particular on the effect parameter δ . We also investigate to what extent the estimation results change if we do not use the multiple spell character of some observations.

The specification of the sanction effect may be incorrect to the extent that δ may vary over the population. Such a specification error can be interpreted as variation of a parameter over the population. A suitable test is the information matrix (IM) test developed by White (1982) and simplified by Lancaster (1984). Chesher (1984) shows that this test can be applied to testing for parameter heterogeneity. Basically, the IM test compares elements of two different estimates of the information matrix, based on individual likelihood score and Hessian contributions, respectively. Both estimates are asymptotically equivalent under the null hypothesis that no misspecification occurs. Significant differences can be attributed to misspecification, or more specifically, in Chesher's interpretation, to variation of specific parameters over the population.

Applied to δ the IM test boils down to comparing estimates of the entry of the information matrix belonging to δ . IM statistics for δ are χ_1^2 distributed and equal 2.41 and 2.84 for the metal industry and banks, respectively. Clearly, the null hypothesis that there is no additional parameter heterogeneity in δ cannot be rejected.

We also investigate the variation of the effect parameter δ in a different way, namely by specifying and estimating δ as a function of explanatory variables x : $\delta = \delta(x) = x'\gamma$, for some parameter vector γ that replaces the single effect parameter δ . As explanatory variables x we introduced a constant, age, age squared, wage and wage squared. Table 4 gives estimates of the parameter vector γ . Estimates of the other parameters are omitted as those are virtually the same as the estimates reported in Tables 2 and 3. Table 5 presents loglikelihood values of the basic model and some alternative specifications. The constant terms reported is adjusted for the introduction of explanatory variables and comparable to the basic estimates of a constant δ . Clearly, the mean effects of sanctions do not change much. Thus, the 'average level' of the effect of sanctions is stable under introduction of explanatory variables. For the metal industry none of the other coefficients of δ differs significantly from zero, and standard errors are large. Furthermore, the LR test statistic equals 3.32, indicating that it is

allowed to restrict the coefficients of the explanatory variables to zero. For the banking sector we find a significant negative effect of wage and a significant positive effect of wage squared, indicating that the effect of a sanction declines with the wage, but the decline is less than proportional. A LR test (7.64) shows that the hypothesis that the coefficients of the explanatory variables all equal 0 cannot be rejected. Thus, the LR results for both the metal industry and the banking sector are in line with the IM test results, again confirming that the sanction effect is specified correctly.

It is possible that sanctions have an effect only shortly after they have been imposed. This would mean that the effect of a sanction would vanish after a while. To investigate the possibility that this effect exists we introduced duration dependency in the effect parameter by allowing this parameter to change values around 8 weeks after the imposition of a sanction. From additional estimates it appears that the changes in δ after 8 weeks are minor and insignificant: the metal industry δ increases by 0.13 after 8 weeks (standard error 0.23), the banking sector δ decreases by -0.22 (standard error 0.24). Apparently, the value of the effect parameter does not change over time. From Table 5 it appears that the LR statistics equal 3.14 and 3.98, which implies that lack of duration dependency of δ cannot be rejected for the metal sector. For the banking sector, the LR test and the Wald test are in conflict, so that the evidence is inconclusive.

Finally, we investigate the robustness of our results by using single instead of multiple spell observations. If we delete second and higher spells, the number of individuals equals the number of spells. The estimated coefficients of the single spell model are virtually the same as those from the multiple spell model. Thus, we conclude that our results are not heavily dependent on the assumptions made on the multiple spells.

All in all, we conclude that our estimation results are very robust.

5 Simulations

In this section we present simulations to shed some light on the magnitude of various effects that play a role in our model. We first define some useful quantities for simulations.

First, consider the effect of imposition of a sanction on the probability of becoming a long term unemployed. More precisely, define the probability of becoming long term unemployment as the probability that the process of re-employment takes more than k weeks, for some $k > 8$. We can compute such probabilities both given that no sanction is imposed and conditional on imposition of a sanction at

8 weeks. The difference between both probabilities is indicative of the effect of sanctions in a way that appeals to the everyday observer of unemployment. It simply says by how much the probability of long term unemployment can be reduced by imposing a (punitive) reduction after 8 weeks of unemployment.

The probability of long term unemployment is, conditional on v_u , x , and conditional on that no sanction is imposed before k , given by $S_{u,1}(k|x, v_u)$. If however a sanction is imposed at 8 weeks of unemployment, the relevant conditional probability is $S_{u,2}(k|x, v_u, t_s = 8)$.

Second, we can consider the conditional probability that a sanction is imposed during a complete unemployment spell. Note that this exceeds the observed incidence of sanctions due to right-censoring in the data. The probability of imposition of a sanction can be expressed as

$$\Pr(t_u > t_s | x, v_u, v_s) = \int_0^{\infty} \theta_s(t|x, v_s) S_{u,1}(t|x, v_u) S_s(t|x, v_s) dt \quad (32)$$

These expressions allow us to compute long term unemployment risks and sanction probabilities conditional on v_u , v_s and x . In order to establish the effects of the individual elements of x we integrate the expressions above with respect to the suitable unobserved heterogeneity distributions.

Tables 6 and 7 present computed values of the survival probabilities at 26 and 52 weeks and the probability of imposition of a sanction, conditional on various observed and unobserved characteristics. For both survival probabilities at 26 and 52 weeks there are two columns, the first being the survival probability if no sanction is imposed, the second being the survival probability if a sanction is imposed after 8 weeks. The first row contains unconditional statistics computed in the sample mean of (the linear components of) x , which by construction equals 0. These statistics serve as a baseline. The second up to the fifth row contain the same statistics conditional on the various realizations of v , again computed in the sample mean of x . Finally, we computed unconditional statistics by allowing x to deviate from the sample mean by one characteristic at a time. These are tabulated in the sixth up to the last row. For the dummy characteristics statistics for the various prevailing values of these dummies are tabulated. Young (old) unemployed are individuals that are 10 years younger (older) than the mean sample age. Low (high) wage individuals are individuals with daily wages 50 guilders below (over) the average wage.

As was to be expected from the discussion of the parameter estimates in the last section, the simulations deliver simultaneous variation in survival probabilities and the probability of imposition of a sanction. Again, this stresses the importance of correcting for unobserved and observed selectivity biases.

Table 6 shows that in the metal industry sample if no sanction is imposed the average survival probability is 57% after 26 weeks and 44% after 52 weeks. If a sanction is imposed after 8 weeks the average survival probability is 46% after 26 weeks and 28% after 52 weeks. Therefore, the imposition of a sanction has quite an effect on the outflow out of unemployment to a job.

On average, during 7% of the unemployment spells a sanction is imposed. These statistics vary greatly over the four unobserved groups. Some 3% of the sample has a probability of 45% of having a sanction imposed. There is also a group of 4% of the sample for which sanctions appear to be absent. The majority of metal industry individuals (88%) combines relatively low re-employment and sanction rates, and face a 6% sanction probability. Clearly, a lot of the variation in both hazard rates is due to unobserved heterogeneity. Some observed characteristics, however, also have considerable impact on these statistics. Older metal workers combine high survival probabilities with high probabilities of having sanctions imposed. Low wage workers have lower transition rates to employment, but sanction rates approximately equal to that of high wage unemployed. Males have low survival probabilities and have about the same sanction probabilities as females. Region is hardly affecting the transition from unemployment to employment, but unemployed in the Southern part of the country have sanctions imposed sooner as other workers. For that reason, individuals living in the South face an 11% probability that a sanction is imposed, whereas in the other areas this probability is only 5-6%. Metal industry unemployed living in urban areas have slightly higher survival probabilities than non-urban unemployed, but sanction probabilities are much higher for this group. Finally, both full time and unmarried unemployed have somewhat higher sanction probabilities than their counterparts.

Table 7 shows the simulation results for banking. It appears that in the banking sector subsample if no sanction is imposed the average survival probability is 44% after 26 weeks and 31% after 52 weeks. If a sanction is imposed after 8 weeks the average survival probability is 25% after 26 weeks and 12% after 52 weeks. Therefore, also for the banking sector the imposition of a sanction has quite a large effect on the outflow out of unemployment to a job. A peculiarity of the banking sector is that there is a subsample of 5% for which the exit rate to a job is zero, so they stay unemployed for ever. The effects of the other variables are quite similar to those of the metal industry. We find that older, low wage, female and urban workers have a lower re-employment probability than their counterparts. The effect of region, marital status and whether a worker had a part time or a full time job is very small. When it comes to the size of the re-

employment hazard rate age has the largest effect. The probability to find a job within a year without a sanction being imposed is about 80% for young workers, while it is about 50% for old banking sector workers and about 25% for old metal industry workers. The variation of sanction probabilities over different groups in the banking sample resembles the variation in the metal industry sample, but is slightly less pronounced.

6 Conclusion

In this paper we analyze the effect of unemployment insurance sanctions on the transition rate from unemployment to employment. We use a unique data set of administrative micro data with information on unemployment durations and sanctions that are given during a spell of unemployment. A potential problem concerns the selectivity of the imposition of sanctions on the unemployed. As transition rates of unemployment to employment and the imposition of sanctions may be dependent, results from univariate duration analysis with sanction as an exogenous ‘treatment’ dummy may be biased. We deal with this problem by simultaneously modeling the process by which unemployed get sanctions and the process by which they find jobs. We estimate bivariate duration models with both observed and, possibly correlated, unobserved heterogeneity. Furthermore, we allow for a flexible form of duration dependence of the hazard rates.

The main result of this analysis is that transition rates to employment are significantly and substantially raised by imposition of a sanction. Thus, we conclude that sanctions are effective. Furthermore, our analysis shows that the sanction rate and the transition rate to employment are simultaneously affected by various observed characteristics and by unobserved characteristics. So, we may also conclude that univariate analyses would lead to biased measurement of the effect of sanctions.

As argued in the theoretical section, the sanction effect can be thought of being due to two changes on a deeper level: (i) the decrease in the UI benefits level, and (ii) the increase in the search intensity. According to the theoretical model, the effects of these changes are not additive: they may reinforce each other in their effects on the hazard, and, indeed, it is unlikely that the search intensity effect is positive while at the same time the benefits effect is absent. It is thus plausible that the benefits change plays a significant role. If we would assume that *all* of the sanction effect is due to a benefits change of -20%, then the absolute value of the elasticity of the transition rate w.r.t. benefits, as implied by the estimate of δ , would exceed 3. Clearly, a sanction is more than just a benefits

change, but it seems unlikely that such a considerable effect can be obtained without any benefits effect.

This means that our results are in contrast to most existing studies dealing with benefits effects on unemployment duration in the Netherlands, in the sense that those studies consistently found no benefits effect (see e.g. Van den Berg (1990b) for a survey). There are three explanations for this. First, previous studies did not deal with endogeneity of the benefits level or changes of this level. This may bias the results in those studies. Secondly, the benefits change associated with a sanction is quite substantial. The latter may reduce the scope for income smoothing to deal with the shock in income. In other words, the benefits elasticity may be nonlinear in the size of the change of the benefits level. Thirdly, the benefits elasticity may be larger in the nineties than in the previous decades.

As a topic for further research, it may be attempted to combine the present data with micro data on UI recipients, in order to exploit micro information on the magnitude and duration of temporary sanctions. Ideally, micro data containing information on search activities can also shed light on the extent in which sanctions affect search intensity.

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Appendix: The data set

The data set is provided by Dutch Social Security Council (SVr) and are administrative data from the sectoral organizations that implement the unemployment insurance system. All cases of individuals applying for unemployment benefits in 1992 were included in the database, and, if necessary, followed up to September 1993. We excluded all cases that started collecting benefits before 1992 or had sanctions imposed right at the start of the benefits period. Furthermore, we excluded cases for which one or more exogenous variables are missing. After selection of the data of the three occupational organizations used in this paper, we are left with the number of spells and individuals denoted in Table 1. Below we give some details on measurement and construction of some of the variables.

1. duration unemployment insurance benefits:

Both the duration of the insurance benefits period and the destination state of individuals whose benefits expire are observed. Durations are observed in intervals. 13 bi-weekly intervals cover the first half year. Then we have one 6-week interval, for durations between 26 and 32 weeks. On the interval 32 to 318 weeks we are able to distinguish 22 quarterly duration classes. The remaining durations are observed as being 318 weeks or longer. As we are not considering benefit payments that started before 1992, and we are only following benefits recipients up to September 1993, there is no right-censoring because of observations in the residual class 318 weeks and higher. We observe, however, unemployment spells that are still lasting at the end of September 1993, and destinations of transitions out of unemployment insurance different from employment. In our analysis, both are considered to be right-censoring.

2. sanctions:

The duration of unemployment on the moment a sanction is imposed is observed in the interval described above. Furthermore, it is known whether a sanction is imposed right at the start of the benefits period or during the period of benefits reception. As argued, these are fundamentally different types of sanctions. We have deleted cases that have sanctions imposed right at the start of the unemployment spell.

3. age:

Age is computed as the age in years at the start of the individual's benefits spell.

4. **wage:**

Wage is the daily wage before taxes earned by the individual before becoming unemployed. It is the wage that is used by the administrative organization to compute the level of the benefits (see Section 3). It is observed in 43 intervals of width 10 guilders up to 430 guilders, and a residual interval for those earning over 430 guilders. The continuous wage variable is defined as the average wage in each wage class, or 435 guilders for those in the highest wage class.

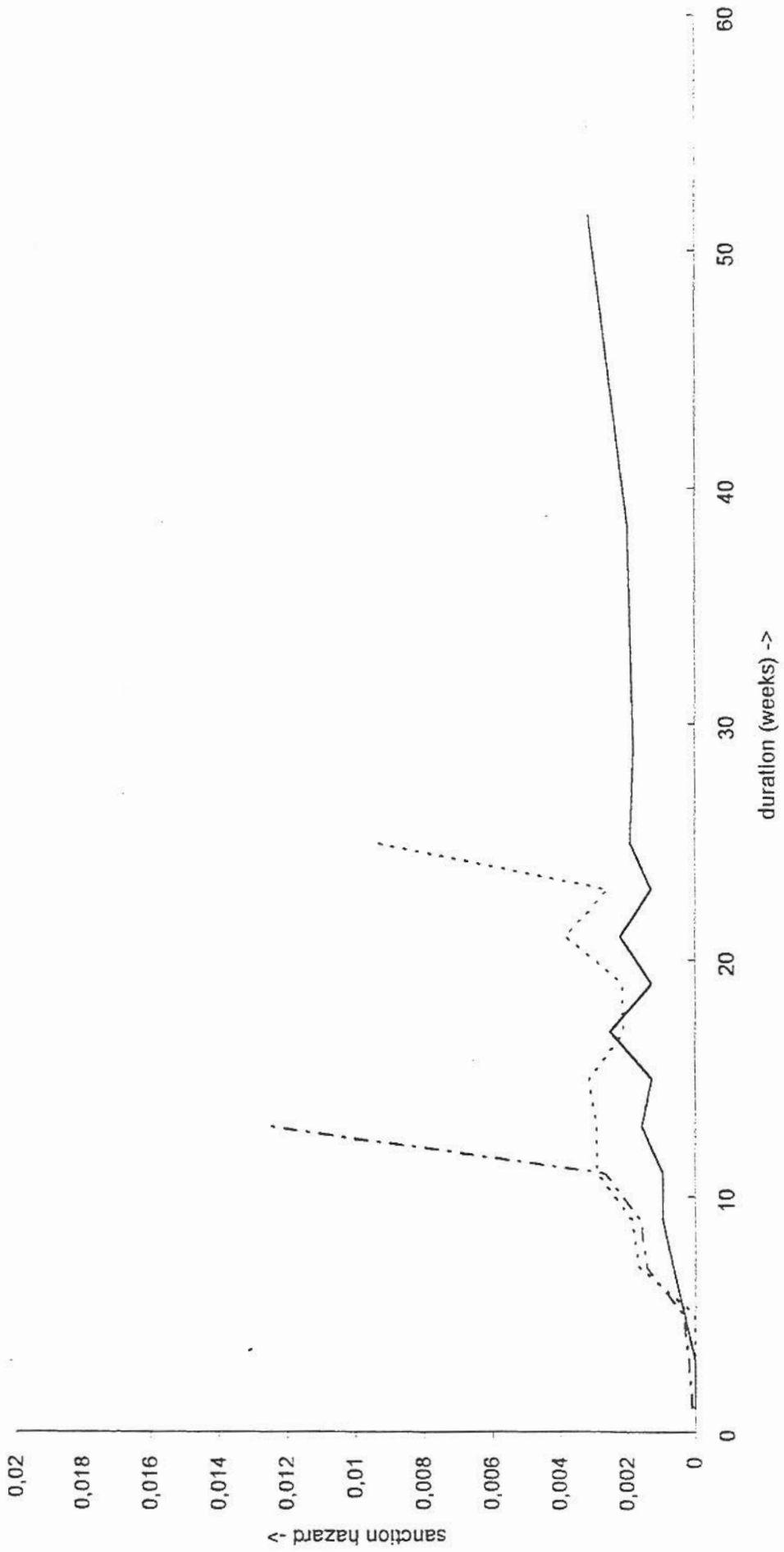
5. **region:**

municipality codes are observed and recoded to regional and urbanization dummies. The north is defined as the provinces of Groningen, Friesland, and Drenthe. The east consists of Overijssel, Flevoland, and Gelderland. The south contains Noord-Brabant and Limburg. The west represents Utrecht, Noord-Holland, Zuid-Holland, and Zeeland. Urbanized areas are municipalities that are highly urbanized according to the Dutch Central Bureau of Statistics (CBS): Amsterdam, Delft, The Hague, Groningen, Haarlem, Leiden, Rijswijk, Rotterdam, Schiedam, Utrecht, Vlaardingen, and Voorburg.

6. **part time/full time:**

Like the wage information this variable refers to the employment situation of the benefits recipient preceding the unemployment spell. Full time refers to working 100% or more of the regular number of hours. Part time refers to working less than 100% of the regular number of hours.

Figure 1. $\theta_s(t_s | x, t_u)$



..... given $12 \leq T_u < 14$ ——— given $24 \leq T_u < 26$ - - - - given $45 \leq T_u < 58$

Table 1: Sample statistics

	metal industry	banking sector
age (years)	38.0	34.3
wage (guilders)	162.6	152.2
female	26.2%	47.7%
male	73.8%	52.3%
north	13.1%	7.9%
east	21.0%	17.4%
south	35.3%	22.6%
west	30.6%	52.0%
urban	9.5%	19.2%
not urban	90.5%	80.8%
part time	10.5%	18.2%
full time	89.5%	81.8%
married	55.5%	46.9%
not married	44.5%	53.1%
# spells	7,758	32,341
# individuals	6,729	25,934
sanctions	2.1%	2.4%

Table 2: Metal industry

	θ_u	θ_s
v^a	-3.73 (0.06)	-5.61 (1.23)
v^b	-1.71 (0.14)	-8.36 (0.41)
δ	0.57 (0.17)	
age	-0.97 (0.03)	-0.82 (0.13)
age squared	-0.25 (0.02)	-0.29 (0.10)
wage	0.55 (0.05)	-0.13 (0.25)
wage squared	-0.21 (0.03)	-0.09 (0.15)
female	-0.30 (0.05)	-0.39 (0.21)
north	-0.05 (0.07)	0.00 (0.40)
east	0.02 (0.06)	0.33 (0.34)
south	-0.05 (0.06)	0.97 (0.30)
urban	-0.13 (0.08)	0.62 (0.36)
part time	0.03 (0.08)	-0.49 (0.36)
married	-0.14 (0.05)	-0.77 (0.20)
λ_1	-0.32 (0.05)	1.21 (0.27)
λ_2	-0.52 (0.07)	
λ_3	-0.70 (0.08)	
λ_4	-0.94 (0.10)	
λ_5	-1.24 (0.14)	
λ_6	-1.06 (0.17)	
p_1	0.03 (0.05)	
p_2	0.05 (0.08)	
p_3	0.88 (0.05)	
p_4	0.04 (0.08)	
$\rho(v_u, v_s)$	0.59 (0.50)	
# spells	7,758	
# individuals	6,729	
log \mathcal{L}	-12,472.66	
(full model)	-12,469.79	
IM-statistic δ	2.41	

Explanatory note: All exogenous variables are included in deviation from their sample means, except age squared and wage squared. Age squared and wage squared are age and wage in deviation from their sample means raised to the square. Standard errors are given in parentheses. "Full model" is the model without the restrictions on λ_s .

Table 3: Banking sector

	T_1, T_2	T_s
v^a	$-\infty$	-7.69 (0.57)
v^b	-3.15 (0.04)	-8.75 (0.42)
δ	0.81 (0.18)	
age	-0.45 (0.03)	-0.45 (0.15)
age squared	-0.04 (0.02)	-0.07 (0.10)
wage	0.19 (0.04)	-0.59 (0.23)
wage squared	-0.06 (0.02)	0.15 (0.13)
female	-0.18 (0.04)	-0.33 (0.25)
north	-0.17 (0.08)	-0.03 (0.41)
east	-0.11 (0.06)	0.02 (0.30)
south	-0.06 (0.05)	-0.27 (0.30)
urban	-0.29 (0.06)	0.21 (0.28)
parttime	-0.07 (0.06)	0.00 (0.31)
married	-0.09 (0.05)	-0.16 (0.24)
λ_1	-0.19 (0.05)	1.97 (0.41)
λ_2	-0.42 (0.06)	
λ_3	-0.88 (0.09)	
λ_4	-0.96 (0.10)	
λ_5	-1.31 (0.15)	
λ_6	-1.11 (0.18)	
p_1	0.05 (0.00)	
p_2	0.95 (0.00)	
$\rho(v_u, v_s)$	-1	
# observations	4,826	
# individuals	3,862	
log \mathcal{L}	-10,305.94	
(full model)	-10,303.52	
IM-statistic δ	2.84	

Explanatory note: All exogenous variables are included in deviation from their sample means, except age squared and wage squared. Age squared and wage squared are age and wage in deviation from their sample means raised to the square. Standard errors are given in parentheses. Missing standard errors indicate that the accompanying parameters are fixed at boundary values. "Full model" is the model without the restrictions on λ_s .

Table 4: Estimates parameterized δ

	metal industry	
δ^*	0.69	(0.60)
age	-0.08	(0.45)
age squared	-0.07	(0.29)
wage	0.29	(0.39)
wage squared	0.32	(0.46)
# observations	7,758	
# individuals	6,729	
$\log \mathcal{L}$	-12,471.00	
	banking sector	
δ^*	0.59	(0.25)
age	-0.25	(0.27)
age squared	0.22	(0.23)
wage	-0.85	(0.32)
wage squared	0.53	(0.20)
# observations	4,826	
# individuals	3,862	
$\log \mathcal{L}$	-10,302.12	

Explanatory note: δ is parameterized by $\delta(x) = x'\gamma$, where x has columns with ones, age, age squared, wage, and wage squared. The regressors are specified as in the basic model. Therefore, $\delta^* = x'\hat{\gamma}$ evaluated at the mean age and wage is reported instead of $\hat{\gamma}_1$, the first element of the estimate $\hat{\gamma}$ of γ , belonging to the constant in x . δ^* can directly be compared to δ in the basic model. Note that $\delta^* = \hat{\gamma}_1 + \hat{\sigma}_a^2 \hat{\gamma}_3 + \hat{\sigma}_w^2 \hat{\gamma}_5$, where $\hat{\gamma}_i$ is the i -th element of $\hat{\gamma}$, and $\hat{\sigma}_a^2$ and $\hat{\sigma}_w^2$ are the sample variance of age and wage, respectively.

Table 5: Sensitivity of the estimation results: log likelihood values for various specifications

	df	metal industry	banking sector
basic model	0	-12,472.66	-10,305.94
full model	+5	-12,469.79	
	+7		-10,303.52
parameterized δ	+4	-12,471.00	-10,302.12
duration dependent δ	+1	-12,471.49	-10,303.95

Explanatory note: the second column, labeled “df”, gives the degrees of freedom relative to the basic model.

Table 6: Metal industry

	$\Pr(T_u > 26)$		$\Pr(T_u > 52)$		$\Pr(T_s < T_1)$
	$t_s = \infty$	$t_s = 8$	$t_s = \infty$	$t_s = 8$	
sample mean	0.57	0.46	0.44	0.28	0.07
(v_u^a, v_s^a)	0.62	0.50	0.49	0.31	0.45
(v_u^b, v_s^a)	0.03	0.01	0.00	0.00	0.04
(v_u^a, v_s^b)	0.62	0.50	0.49	0.31	0.06
(v_u^b, v_s^b)	0.03	0.01	0.00	0.00	0.00
young	0.35	0.22	0.21	0.08	0.05
old	0.82	0.76	0.75	0.65	0.10
low wage	0.65	0.56	0.54	0.39	0.10
high wage	0.50	0.39	0.37	0.21	0.05
female	0.63	0.53	0.51	0.35	0.07
male	0.55	0.43	0.42	0.25	0.07
north	0.58	0.47	0.45	0.29	0.05
east	0.56	0.45	0.43	0.27	0.06
south	0.58	0.47	0.45	0.29	0.11
west	0.56	0.45	0.44	0.27	0.05
urban	0.60	0.50	0.48	0.32	0.12
not urban	0.57	0.45	0.44	0.28	0.07
part time	0.56	0.45	0.44	0.27	0.05
full time	0.57	0.46	0.45	0.28	0.07
married	0.59	0.48	0.46	0.30	0.06
not married	0.55	0.43	0.42	0.25	0.09

Explanatory note: Computations are conditional on the characteristics in the left column. All other observed characteristics are fixed at their sample means and, if appropriate, the expected value with respect to v is taken.

Table 7: Banking sector

	Pr($T_u > 26$)		Pr($T_u > 52$)		Pr($T_s < T_1$)
	$t_s = \infty$	$t_s = 8$	$t_s = \infty$	$t_s = 8$	
sample mean	0.44	0.25	0.31	0.12	0.09
(v_u^a, v_s^a)	1	1	1	1	1
(v_u^b, v_s^b)	0.41	0.21	0.27	0.07	0.04
young	0.30	0.14	0.18	0.07	0.08
old	0.60	0.41	0.48	0.24	0.10
low wage	0.48	0.28	0.35	0.14	0.11
high wage	0.41	0.22	0.28	0.11	0.08
female	0.47	0.28	0.34	0.14	0.09
male	0.41	0.22	0.28	0.10	0.09
north	0.48	0.29	0.35	0.14	0.10
east	0.46	0.27	0.33	0.13	0.10
south	0.44	0.25	0.31	0.12	0.08
west	0.42	0.23	0.29	0.11	0.09
urban	0.52	0.32	0.39	0.17	0.11
not urban	0.42	0.23	0.29	0.11	0.08
part time	0.46	0.26	0.33	0.13	0.09
full time	0.44	0.24	0.31	0.12	0.09
married	0.46	0.26	0.33	0.13	0.09
not married	0.42	0.23	0.30	0.11	0.09

Explanatory note: Computations are conditional on the characteristics in the left column. All other observed characteristics are fixed at their sample means and, if appropriate, the expected value with respect to v is taken.